

From: [Jay Field](#)
To: [PETERSON Jenn L](#)
Cc: [Eric Blischke/R10/USEPA/US@EPA](#); [pj.bridgen@eilttd.net](#); [jeremy_buck@fws.gov](#); [Joe Goulet/R10/USEPA/US@EPA](#); [Burt Shephard/R10/USEPA/US@EPA](#); [Robert.Neely@noaa.gov](#); [OMFALY Mikell](#); [chris.thompson@eilttd.net](#); [Chip Humphrey/R10/USEPA/US@EPA](#)
Subject: Re: Summary of the November 21st Benthic Meeting
Date: 12/08/2005 12:10 PM
Attachments: [wr_chems_n_qual.xls](#)

attached file is same as before but has units included.
Jay

Jay Field wrote:

> Jenn,
> thanks for your comments (and a copy of the memo from Windward). A
> couple of quick comments to the memo. As you pointed out, Windward
> assumed agreement about several issues that I did not think were
> resolved in the meeting:
> 1) FPMModel endpoints: I do not think we agreed that the FPM should
> use only the individual endpoints. We certainly did not agree that
> the FPM should ignore the Hyalella growth endpoint. Poor model
> performance is not a good reason to ignore the most sensitive
> endpoint. As we pointed out in the July meeting, in the memo to LDW,
> and again in the 11/21 meeting, the pooled growth/survival results for
> the each species is a better way to look at the growth results because
> growth is not independent of survival. If they insist on using the 4
> individual endpoints, I think it is reasonable to ask them to apply
> the FPM to the pooled results for each species as well.
>
> 2) control-normalized results: I still do not agree with Teresa's
> approach to control-normalization (subtracting the control result from
> the test result rather than dividing test by control as is commonly
> done). She also appears to be using this approach for growth (which I
> think is different than in their original benthic methods memo).
> Teresa is following (creating) her own precedent. However, I do agree
> that the results for this data set are likely to be minimal.
>
> 3) N-qualified data: attached is a spreadsheet summarizing (min, max,
> and number of samples) the N-qualified results for each chemical.
> Note that there are some high concentrations that would be excluded
> for a variety of chemicals. I have not been excluding them in my
> analyses (I apparently missed the discussion of this qualifier).
>
> 4) summing chemicals: I am using total PCBs. Because of the strong
> correlation between individual PAHs and LPAH and HPAH, I do not think
> it's a significant problem to use LPAH & HPAH instead of the
> individual PAHs. I have not looked at the relationship between total
> DDTs and the six isomers. If they use the summed values for PAHs and
> DDTs, they should show the basis in the report (consistent composition
> throughout the study area).
> 5) in my view, the most critical issue at this time is to resolve Mike
> A's issues on the FPM, to make sure that the final model results are
> reproducible.
>
> Jay
>
>
>
> PETERSON Jenn L wrote:
>
>> I quickly looked this over, and had a few comments (enclosed with my
>> comments added in). Also, there were some things brought up at the
>> meeting for agreement (I think) that are not mentioned here and I am not
>> sure we agreed with. Overall, here are some of the issues I see for the
>> modeling effort. I briefly summed up some of the technical issues I
>> have, both technical and larger issues. Anyone please jump in, dismiss
>> or clarify my issues, or add to this summary!
>>
>>
>> Technical Issues:
>>
>> 1. Summing contaminant classes versus using individual contaminants in
>> the model: The proposal from the LWG was to sum DDTs, PAHs and PCBs (I
>> think that was all). However, Mike and Jay are currently not summing.
>> It seems better to sum at a later stage. For example, with the FPM Mike
>> is running, you can see if contaminants are correlated and maybe should
>> be summed from the results of the analysis. If contaminants are
>> co-varying they will show this by where they "float" in the analysis".
>> If this is shown, summing at that point makes sense, but maybe not
>> before. It would be better to present unsummed analysis and summed -
>> that way we have the information we need to make a decision on what
>> numbers are more appropriate.
>> 2. Alpha levels: In the meeting they mentioned they were running the
>> analysis using an alpha level of 0.05. The alpha levels represent the
>> probability of making incorrect conclusions that the treated sample is
>> toxic or contains chemical residues not found in the control or
>> reference sample (Type 1 error). By setting this probability low
>> (0.05), the likelihood that one erroneously concludes there are no
>> differences among the mean responses in the treatment, control or
>> reference samples (Type 2 error) increases (low power). Type 2 errors
>> would lead to conclusions that the sample is not toxic (or different
>> from control or reference), when in fact there is a difference. Type 2
>> errors are important to minimize in environmental investigations, since,
>> if left undetected, these errors can lead to continued short- and

>> long-term effects (ASTM 2003; EPA 2000a). In order to avoid this, an
>> alpha of 0.1 can be used (and is in the work plan), which would increase
>> the power of the test and the probability of detecting a reduction
>> relative to the control mean. They are currently eliminating some
>> samples on the basis that they are indeterminate in difference from the
>> control at an alpha of 0.05 (I think from the meeting there were about
>> 11 eliminated). However, they may be determinate at an alpha of 0.1.
>> These low responses may be important in the model - especially the FPM.
>> In the work plan they state "if the analysis of the toxicity test data
>> finds that the power for the data set is low, the alpha level may be
>> raised to 0.1 as suggested in ASTM guidelines (2003)." From the meeting
>> there was not mention they were moving forward with that analysis,
>> however, I would recommend the report should include the analysis at an
>> alpha of 0.1 and indicates how this changes the conclusions.

>> 3. What contaminants should be eliminated from the model: This relates
>> to removing contaminants on the basis that they are not drivers of
>> toxicity (e.g. aluminum). However, Mike A's analysis showed that some
>> were slight predictors of toxicity. It may still be removed later on
>> the basis that it is not a toxicity driver, but the report, (and their
>> analysis) should include these contaminants (see "3" below). The
>> analysis (at least for the FPM) will clearly show contaminants that
>> aren't driving toxicity, and this will provide justification for
>> dropping contaminants.

>> 4. The results of the bioassay tests and modeling effort may show that
>> additional lines of evidence may be important in interpreting the
>> bioassay results (e.g. EqP or pore water testing).

>>
>>
>> Larger Issues Include (may need more manager input):
>>
>> 1. Running the FPM - there are still discrepancies between Teresa and
>> Mike's models that must be resolved at a fundamental level. We don't
>> want to be dealing with problems in replicating the FPM further down the
>> line when we are also having to analyze results. I would recommend that
>> these issues be worked out prior to submittal of the report, but more
>> importantly that ALL steps she takes to get the FPM values be explicitly
>> written out for each chemical / decision made. This should be at the
>> detail that someone reading the report can replicate what was done.

>> 2. Discrepancies between the FPM and the logistic regression results:
>> PAHs are a good example of this. The FPM method is calculating very
>> high dry weight concentrations of PAH threshold numbers using this
>> method that the government team does not agree with (and Jay has said is
>> a non-starter).

>> 2. What endpoints should we be considering? The Hyalella growth
>> endpoint appears to be producing different results than the other test
>> endpoints. Teresa wants to remove this from her analysis because it is
>> not producing reliable results, even though it is being used as a part
>> of the logistic regression modeling. I don't think the team members
>> agree with this assessment. I would recommend model runs for this
>> endpoint should be included in the report, along with pooled endpoint
>> runs that include this endpoint. We can then assess what it means after
>> we see the data.

>> 3. What do we want the models to do? Loraine brought up this point and
>> it is a very good one. Do we want the model to provide information on
>> the chemicals detected in Portland Harbor or find the most predictive
>> component that is predictive of toxicity (e.g. even if it is a
>> conventional parameter)? You can run the models and get numbers for
>> each chemical - if it is not contributing to toxicity this number will
>> most likely be the AET from the dataset. However, I think this is
>> useful information to anyone reviewing this report. I would recommend
>> that most chemicals be run in order to justify their removal (which is
>> easy running the FPM, but maybe not the logistic regression). Mike
>> Anderson did this very quickly, and showed that some chemicals were not
>> contributing to toxicity on the basis of the analysis. Numbers behaving
>> in this manner were flagged with an AET value. By doing this it is easy
>> to see that contaminant X wasn't a driver for toxicity at the highest
>> detected concentration of X. This information is useful. The
>> alternative is to find the most predictive indicator of toxicity, which
>> may be a conventional parameter such as bulk ammonia, bulk sulfide or
>> percent fines, or it may include a very limited list of contaminants.
>> The downside here is that this approach may provide limited data on a
>> wider list COPCs. If we go this route, bioassays to validate the model
>> should definitely be done, and realize that it will not translate easily
>> into cleanup numbers.

>> 4. What hit/no hit thresholds should we be considering? We gave some
>> direction in our memo to them. However, they resisted going to the same
>> thresholds between methods (for the FPM) in order to comply with
>> consistency with other programs (which is odd because the "other
>> programs" are still Teresa's work, but for Washington State). We had
>> originally proposed using 10, 20 and 30 (or 90, 80 and 70) to correspond
>> with NOAA's levels. Teresa did stat only, 10 and 25 for Washington
>> State. Therefore, we got pushback on using the NOAA thresholds for
>> Teresa's FPM analysis. Jay seems to think this is o.k. because the
>> threshold levels don't matter too much as long as you get information at
>> several levels for the model. I agree with him for the logistic
>> regression model (because eventually you are developing a continuous
>> model for which you can pick anywhere on the curve to correspond with
>> magnitude of toxicity and prob of toxicity [jay correct me if I am
>> wrong] for use in management objectives), but this is not the case for
>> the FPM. Magnitude of toxicity (hit/no hit) levels need to be selected
>> before hand and that is all the data you will have to make decisions.
>> You can't for example select another threshold (e.g. something between
>> the 10 and the 25) without re-running the analysis because you do not
>> have a continuous distribution like the logistic regression model. We
>> concluded that because of the resistance and since Mike had the data he
>> could run the 10, 20, and 30 for the government team and we could
>> analyze any differences between the different levels of magnitude of

>> toxicity. However, it would have been better to stay consistent, and I
>> think the three levels indicating magnitude of toxicity would have been
>> helpful in interpreting the data for the FPM.

>> -Jennifer

>> -----Original Message-----
>> From: Blischke.Eric@epamail.epa.gov
>> [mailto:Blischke.Eric@epamail.epa.gov] Sent: Wednesday, December 07,
>> 2005 12:13 PM
>> To: pj.bridgen@envintl.com; jeremy_buck@fws.gov;
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>> chris.thompson@eiltld.net
>> Subject: Fw: Summary of the November 21st Benthic Meeting

>> Attached is the meeting summary that we discussed this morning.

>> Eric
>> ----- Forwarded by Eric Blischke/R10/USEPA/US on 12/07/2005 12:12 PM
>> -----

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>> Subject: Summary of the November
>> 21st Benthic
>> Meeting

>> Hi Joe, attached is our write-up of the summary of the meeting. Let me
>> know if you want to add, edit, delete action items, etc. Also, let me
>> know if you want to have a conference call on any of the issues (outside
>> of the Teresa/Mike calls and the Lorraine/Jay calls). We are moving
>> forward and are targeting a early Feb submittal date for the report.
>> Thanks, Lisa

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>> electronic transmission in error, please notify me by telephone at
>> (206)577-1288, or by electronic mail, lisas@windwardenv.com.

>> (See attached file: Summary of Nov21 Benthic Mgt.doc)

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